# Double Impact: What Sibling Data Can Tell Us about the Long-Term Negative Effects of Parental Divorce*

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ABSTRACT: **Most prior research on the adverse consequences of parental divorce has analyzed only one child per family. As a result, it is not known whether the same divorce affects siblings differently. We address this issue by analyzing paired sibling data from the 1994 General Social Survey (GSS) and 1994 Survey of American Families (SAF). Both seemingly unrelated regressions and random effects models are used to study the effect of family background on offspring's educational attainment and marital stability. Parental divorce adversely affects the educational attainment and the probability of divorce of both children within a sibship; in other words, siblings tend to experience the same divorce the same way. However, family structure of origin only accounts for a trivial portion of the shared variance in offspring's educational attainment and marital stability, so parental divorce is only one of many factors determining how offspring fare. These findings were unchanged when controlling for a number of differences both between and within sibships. Also, the negative effects of parental divorce largely do not vary according to respondent characteristics.**

## INTRODUCTION

Parental divorce can have lasting negative consequences for adult offspring. This finding has been replicated with many nationally representative data sets, including the National Survey of Family Growth (McLanahan and Bumpass, 1988), the Study of Marital Instability Over the Life Course (Amato and Booth, 1991, 1997), the General Social Survey (Glenn and Kramer, 1985, 1987; Wolfinger, 1998, 1999, 2003a), the National Survey of Families and Households (Amato and Keith, 1991; Wolfinger, 2000, 2003b), and others. These studies share a noteworthy design characteristic. Whether by choice or data limitations, almost all have analyzed one child per family. This leads to a simple but important question: Do the adverse consequences of growing up in a divorced family vary within sibships? This paper explores the negative effects of parental divorce on offspring educational attainment and marital stability using nationally representative sibling data from the 1994 General Social Survey and the Survey of American Families.

Recent scholarly debates on the consequences of parental divorce have brought renewed attention to the question of sibling differences. The much-publicized research of Judith Harris (1995, 1998) contends that the perceived consequences of divorce can be actually be attributed to a combination of genetics and children's peer groups. If peers determine how children fare, there should be considerable variation in how siblings react to parental divorce. The genetic argument is best represented by the work of McGue and

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Lykken, (1992), and Jockin, McGue, and Lykken (1996). Analyses of twins show that the intergenerational transmission of divorce, the increased propensity for the children of divorce to dissolve their own marriages, has a large but not preponderant genetic component. This finding suggests that the effects of parental divorce should be similar within sibships.

Many studies have examined sibling resemblance. Generally their efforts have been unsuccessful—siblings tend to be very different, more so than their shared genetic stock might predict (Dunn and Plomin, 1990). In lieu of genetic explanations, differential socialization can explain some of the variation within sibships. In particular, siblings who are emotionally closer to their mothers and better integrated in their families tend to be better adjusted psychologically following a divorce (Dunn and Plomin, 1990).

Although many researchers have examined the negative effects of parental divorce on offspring, few have considered how the effects of family structure vary within sibships. Moreover, these studies offer contradictory findings. Mekos, Hetherington, and Reiss (1996) compare biological families with stepfamilies comprising sibships with both homogenous and heterogeneous parentage. Siblings within stepfamilies that shared biological parents are similar to one another to the same extent as respondents from intact families, while step-siblings are far more likely to act differently and be treated differently by parents. This result implies that divorce affects siblings similarly, although such a conclusion should be qualified on two grounds. First, it was not the stated purpose of the study. Perhaps remarriage somehow restores similarity among siblings that had been compromised by divorce. Second, Mekos et al. (1996) analyze a nonrepresentative sample of same-sex sibships. Kurdek (1989) also demonstrated a positive association in sibling behavior subsequent to divorce, but analyzed no control group of intact families. Thus it cannot be known whether his results reflect shared influences present prior to the divorce. This limitation also applies to a study by Monahan et al. (1993; see also Buchanan, Maccoby, and Dornbusch, 1996). In contrast to Kurdek (1989) and Mekos et al. (1996) they find little similarity between siblings in divorced families. Studies of twins (Jockin, McGue, and Lykken, 1996; McGue and Lykken, 1992) report considerable similarity in how the children of divorce fare, a result attributed to shared genetic stock. Given that these studies analyze only twins it is unclear whether their results apply to ordinary sibships. The most thorough test of sibling differences was conducted by Sandefur and Wells (1999). Analyzing a nationally representative sample from the National Longitudinal Survey of Youth, they find indirect evidence for similar effects of parental divorce on offspring educational attainment: constraining the effects of family structure on siblings' educational attainment to be equal did not compromise model fit.

One reason the previous sibling studies on the consequences of divorce have reported contradictory findings might be sampling. Only Sandefur and Wells (1999) analyzed a nationally representative sample, while two earlier studies did not include control groups of respondents from intact families. We avoid both of these limitations by analyzing paired sibling data from the 1994 General Social Survey and Study of American Families, the only existing nationally representative sample of adult sibling pairs.

Our analyses extend the results of Sandefur and Wells (1999) in four respects. First, we examine a sample of adults of all ages, whereas Sandefur and Wells (1999) analyzed National Longitudinal Survey of Youth respondents aged 14 to 22 in 1979. A sample thus circumscribed in terms of age and birth cohort may not reflect the experiences of Americans in general. Second, we consider two offspring outcomes, described below; Sandefur and Wells (1999) considered only one. Third, we explicitly address whether the effects of parental divorce vary within sibships; Sandefur and Wells (1999) consider this question only indirectly. Fourth, we expand on Sandefur and Wells (1999) by evaluating the conditions under which siblings may respond differently to the same divorce.

## RESEARCH PLAN

We examine the effects of parental divorce on siblings' educational attainment and divorce histories. These were selected as dependent variables on the basis of both prior studies and availability. Education has been used extensively in prior research on the long-term consequences of parental divorce (e.g., Keith and Finlay, 1988; Krein and Beller, 1988; McLanahan, 1985; McLanahan and Sandefur, 1994). Although a large portion of the relationship between parental divorce and offspring educational attainment can be attributed to differential income and residential mobility while growing up (McLanahan and Sandefur, 1994), these factors should not vary substantially within sibships. The second dependent variable is respondent divorce history. Numerous studies (e.g., Amato, 1996; Amato and Booth, 1991, 1997; Amato and DeBoer, 2001; Glenn and Kramer, 1987; McLanahan and Bumpass, 1988; Wolfinger, 1999, 2000, 2003b) have shown that people from divorced families are disproportionately likely to end their own marriages, probably because parental divorce reduces marital commitment (Amato and DeBoer, 2001).

The GSS contains many other potential outcome variables related to family structure of origin, such as income, occupational prestige, and emotional well-being. However, all suffer the liability of being confounded with other consequences of parental divorce. For instance, parental divorce increases the likelihood of offspring divorce, which in turn may reduce emotional well-being and, for women, income. Occupational prestige is strongly correlated with education. Given the cross-sectional nature of the data it is impossible to disentangle the causal ordering of these outcomes. In contrast, education and divorce history are dependent variables with reasonably clear causal connections to family structure of origin.

Both prior research and common sense provide evidence that the effects of family structure may vary according to respondent demographic characteristics. Glenn and Kramer (1987) found that the transmission of divorce between generations was stronger for whites than for African Americans, and stronger for women than men. McLanahan and Sandefur (1994) showed similar albeit weaker results for the effect of parental divorce on offspring educational attainment. Although the findings on the intergenerational transmission of divorce were not upheld by more recent research (Wolfinger, 1999, 2000, 2003b), their underlying logic merits further scrutiny. Divorce may have more adverse consequences for female offspring because girls often have more difficulty adapting to life in stepfamilies (Hetherington, 1993). Glenn and Kramer (1987) speculated that women contribute more to the maintenance of their own marriages than do men, so the disadvantages conferred by parental divorce may undermine marital stability more for women. Both Glenn and Kramer (1985) and McLanahan and Sandefur (1994) argue that parental divorce may have fewer adverse effects on African Americans because divorce is more institutionalized in Black communities than in their white counterparts. Furthermore, ethnicity appears to be an ongoing issue in the divorce literature: a recent review by Amato (2000) concluded that the number of studies employing adequate samples of non-white families is too small to form firm generalizations. Based on this review, testing for ethnic differences should be an important concern when studying the negative effects of parental divorce on offspring.

Taken together the arguments considered in this paper imply similar ethnic effects within sibships, but different gender effects. All children in a sibship will share the same ethnic influences. On the other hand, girls and boys may react differently to the same set of family conditions. In addition, we test whether the effects of parental divorce vary with the number of additional siblings present. Additional brothers or sisters may provide stability against the tumult of a divorcing family. If this is the case, parental divorce may have fewer negative effects on respondents from large families. Our data allow us to ascertain whether the potential palliative effects of additional siblings apply equally within sibships.

Our overall analytic strategy is to exploit the two important advantages that sibling data offer. Seemingly unrelated regression (SUR) allows us to ascertain whether parental divorce affects siblings differently. Random effects models control for the dependency between siblings (i.e., shared unobserved heterogeneity), thereby permitting us to study the marital stability and educational attainment of each sibling as being conditionally independent of the other.

## METHODS

### Data

We analyze data from the 1994 General Social Survey (GSS) and the Study of American Families (SAF). A national probability sample of English-speaking households within the continental United States, the GSS has been conducted annually or biennially since 1972 (Davis and Smith, 1994). Within each household an adult is randomly selected to be the respondent. The SAF is a survey of randomly selected siblings of 1994 GSS respondents (Hauser and Mare, 1994). The two surveys contain many identical items, so by combining them it is possible to analyze sibling pairs.

This research design presents two potential sources of bias. First, only children obviously cannot be included in an analysis of sibling pairs. If they are particularly sensitive to the effects of parental divorce, parameter estimates would be biased in comparison to results obtained from a conventional sample. However, exploratory analyses using only the GSS data indicate that parental divorce does not affect only children differently. Second, SAF response rates are negatively correlated with parental divorce, resulting in selection bias. Our solution is a two-stage sample selection correction model. We estimate a selection equation where the dependent variable is whether a SAF respondent is available for the substantive regressions (in which education and respondent divorce are the dependent variables). For purposes of identification the selection equation must contain at least one independent variable that does not appear in the substantive equation. This requirement is satisfied with two variables. The first is the percentage of each sibship that is female, given that women generally do a disproportionate share of maintaining kinship ties (Rossi and Rossi, 1990). Thus, heavily female sibships should be in closer overall contact, yielding higher SAF response rates. The second variable measures whether the GSS respondent has moved since age 16, either within or out of state. All else being equal, sibling propinquity should produce higher response rates. Dummy variables measuring family structure of origin are also included as independent variables in the selection equation. Results from the selection equation are then used to generate the inverse of the Mills ratio ($\lambda$), which is included as a regressor in the substantive equations in order to account for the effects of sample selection.

Another data issue concerns the cluster design used to collect the initial GSS sample. Data generated by cluster sampling but analyzed assuming simple random sampling produce standard errors that are biased downward, yielding artificially inflated Z-ratios. A common solution is to calculate Huber-White standard errors (e.g., Greene, 1993), which take the clustering into account. This correction cannot be implemented readily for the random effects models that we estimate. However, preliminary analyses showed that the clustering has no appreciable effect on standard errors calculated from GSS data.

Missing data are minimal and therefore deleted listwise for all variables except parental education (N = 160). For this item we include an additional dummy that measures whether an observation has missing data for parental education. More sophisticated missing data techniques, such as multiple imputation, do not perform appreciably better (Paul, McCaffrey, Mason, and Fox, 2002).

The sample sizes are 1,100 sibling pairs for analyses of education and 404 sibling pairs for analyses of respondent divorce. The sample size for the latter is smaller for two reasons. First, never-married respondents are omitted. Second, the 1994 GSS employed a split ballot design and therefore only half of all respondents were queried about the marital information needed for our analyses. We repeated the analyses of education using only those cases available for the divorce regressions and obtained results similar to those obtained from the full sample.

Table 1, showing summary statistics for all variables, reveals various differences between the GSS and SAF subsamples that at first glance are suggestive of poor data quality. For instance, 26 percent of GSS respondents report the head of their parental family as having less than a high school education, compared to 21 percent of SAF respondents. This probably reflects changes in educational attainment occurring between the times each sibling left the parental household. Residential mobility may account for the different levels of urbanicity reported by siblings (61 percent for GSS respondents, 57 percent for SAF respondents). In contrast, relatively immutable variables such as Catholicism or total number of siblings show almost no difference between the GSS and SAF subsamples. This provides positive evidence about the quality of the data.

TABLE 1

Descriptive Statistics for the Combined Sample, GSS, and SAF Subsamples

| | Total | | GSS | | SAF | |
|---|---|---|---|---|---|---|
| | Mean | SD | Mean | SD | Mean | SD |
| *Parental Family Structure* | | | | | | |
| Intact | 0.76 | 0.43 | 0.77 | 0.42 | 0.76 | 0.43 |
| Divorced mother | 0.07 | 0.26 | 0.07 | 0.26 | 0.07 | 0.26 |
| Remarried mother | 0.02 | 0.15 | 0.02 | 0.15 | 0.02 | 0.15 |
| Other | 0.14 | 0.35 | 0.14 | 0.35 | 0.15 | 0.35 |
| *Race* | | | | | | |
| White | 0.88 | 0.32 | 0.89 | 0.31 | 0.87 | 0.33 |
| Black | 0.08 | 0.27 | 0.08 | 0.27 | 0.08 | 0.27 |
| Other | 0.04 | 0.19 | 0.02 | 0.15 | 0.05 | 0.22 |
| *Parental Education* | | | | | | |
| Not H.S. graduate | 0.24 | 0.43 | 0.26 | 0.44 | 0.21 | 0.41 |
| H.S. graduate | 0.46 | 0.50 | 0.47 | 0.50 | 0.46 | 0.50 |
| Some college | 0.03 | 0.18 | 0.03 | 0.18 | 0.03 | 0.17 |
| College graduate | 0.11 | 0.32 | 0.12 | 0.32 | 0.11 | 0.31 |
| Post graduate | 0.08 | 0.26 | 0.08 | 0.27 | 0.07 | 0.26 |
| Missing data | 0.08 | 0.27 | 0.04 | 0.19 | 0.12 | 0.32 |
| *Other* | | | | | | |
| Ever divorced | 0.33 | 0.47 | 0.36 | 0.48 | 0.30 | 0.46 |
| Years of education | 13.67 | 2.79 | 13.63 | 2.82 | 13.71 | 2.75 |
| Rural | 0.59 | 0.49 | 0.61 | 0.49 | 0.57 | 0.49 |
| Catholic | 0.30 | 0.46 | 0.30 | 0.46 | 0.29 | 0.46 |
| Birth cohort | 49.68 | 15.48 | 49.61 | 15.51 | 49.74 | 15.45 |
| # of siblings | 3.45 | 2.25 | 3.45 | 2.25 | 3.45 | 2.25 |
| Age – age 1st wed | 26.81 | 15.00 | 26.87 | 14.83 | 24.74 | 15.18 |
| Age difference between sibs | 5.17 | 3.96 | 5.17 | 3.97 | 5.17 | 3.96 |
| Male | 0.48 | 0.50 | 0.49 | 0.50 | 0.46 | 0.50 |
| Sibs are same sex | 0.51 | 0.50 | 0.51 | 0.50 | 0.51 | 0.50 |

## Dependent Variables

The first dependent variable is years of completed education, ranging from zero to twenty. Education is treated as continuous to provide greater variability. The second dependent variable is a dichotomy measuring whether or not a respondent has ever been divorced. The coding for these and other variables appears in Appendix A.

## Independent Variables

The GSS and SAF include two items that measure the structure of respondents' families of origin. Respondents were first queried about household composition at the age of 16. If respondents were not living with both biological parents, a second item ascertained the reason. Eighty-five percent of respondents report three varieties of family structure: intact two-parent families, mother-only families resulting from divorce or separation, and mother/step-father families resulting from divorce or separation. Each of the three categories is coded as a dummy variable, with respondents from intact families as the reference category.

An additional dummy is coded for the 15 percent of respondents not falling into the three aforementioned categories. These comprise a variety of other family structures, as well as respondents whose living situations at age 16 were the product of parental military service, parental incarceration, or parental death. These respondents are pooled and included for analytic reasons. Too few people from this diverse group hail from any particular family background to produce individually meaningful parameter estimates, but omission of any respondent falling into the "other" category necessarily means omitting both siblings from the analysis. Doing so could bias the sample, given that individual siblings from "other" family backgrounds are disproportionately likely to have come from divorced families. Retaining "other" respondents and identifying them with their own dummy variable solves this problem. However, these respondents comprise such heterogeneous family backgrounds that it is impossible to draw substantive conclusions about them.

Other independent variables fall into three categories. First, we control for a variety of demographic differences between sibships, including parental education, race, Catholicism at age 16, number of siblings, and rural vs. urban origins. These items allow us to understand whether within-sibship variation on the dependent variables can be explained by sociodemographic differences between families. Two additional variables, respondent education and age at marriage, could potentially be included in analyses of marital stability. They are omitted to preserve consistency with the analyses of educational attainment; moreover, prior research shows that other factors are largely responsible for the relationship between parental divorce and offspring divorce (Amato, 1996; Amato and DeBoer, 2001; Wolfinger, 1999, 2000, 2003b). A second set of independent variables includes characteristics that may vary within sibships. These include respondent gender, whether or not siblings are of the same gender, and the absolute age difference between siblings. In particular, it is important to control for respondent gender when divorce is the dependent variable because men misrepresent their own marital histories more often than women (Bumpass, Martin, and Sweet, 1991). Additional within-sibship measures would be useful, but are not available in the GSS/SAF. Third, we control for birth cohort when education is the dependent variable, given that both average educational attainment and the incidence of parental divorce have changed over time.

An additional variable is necessary for models predicting respondent divorce. Divorce can be right-censored; that is, respondents may still dissolve their marriages after the GSS/SAF interviews. Survival modeling is preferable for this sort of situation, but the GSS/SAF do not provide information on divorce timing and therefore lack adequate data for survival modeling. To contend with right censoring we use a procedure employed by other analysts of GSS data (Glenn and Kramer, 1987; Wolfinger, 1999). A new variable was constructed by subtracting age at first marriage from current age. A lowess model (Cleveland, Grosse, and Shyu, 1992) revealed a curvilinear and non-monotonic relationship (not shown) between respondent divorce and (AGE minus AGEWED), so we modeled the difference between them as a quadratic.

Including this variable in regression equations should largely ameliorate the right censoring bias by modeling the duration of exposure to the hazard of divorce. The two terms of this quadratic are highly correlated with respondent birth cohort ($r = 0.94$ for each), so it is not possible to include both variables in the models predicting divorce.

One liability of the GSS/SAF is the absence of data on respondents prior to parental divorce, given that people from divorced families often exhibit signs of poor well-being even before their parents' marriages end (Cherlin, Chase-Lansdale, and Kiernan, 1995; Cherlin, Chase-Lansdale, and McRae, 1998; Cherlin et al., 1991; Furstenberg and Teitler, 1994; Kiernan and Cherlin, 1999). However, these studies also show that parental divorce affects children irrespective of pre-divorce well-being. The upshot is mostly rhetorical: parental divorce should be thought of as a process, representing an accretion of events, rather than a simple demographic transition.

## ANALYSES

**Seemingly Unrelated Regression (SUR) Analysis.** We use SUR models to see if the effects of parental divorce vary within sibships. In the analysis of educational attainment siblings are randomly assigned to one of two equations that assume the usual ordinary least squares specification:

$$(1a)\ \textit{Education}(1) = \beta_{01} + \beta_{11}(\textit{ParentalDivorce}) + \beta_{21}(\textit{Family}) + \beta_{31}(\textit{Individual}) + \varepsilon_1$$

$$(1b)\ \textit{Education}(2) = \beta_{02} + \beta_{12}(\textit{ParentalDivorce}) + \beta_{22}(\textit{Family}) + \beta_{32}(\textit{Individual}) + \varepsilon_2$$

where Equations 1a and 1b describe the relationship between education and family structure for each of the two siblings. The vectors "Family" and "Individual" respectively represent the familial variables and person-specific independent variables. SUR models provide more efficient estimates of parameters and standard errors when the error terms are allowed to be correlated across equations 1a and 1b (i.e., $r[_1,_2)]\neq 0$). This is assumed to be the case because siblings are affected by the same unmeasured variables. The SUR models also allow formal testing of the hypothesis Ho: $\beta_{11} = \beta_{12}$.

A comparable SUR model is used for the sibling analysis of marital stability, in which divorce is treated as a dichotomous dependent variable. Siblings are randomly assigned to one of two probit equations:

$$(2a)\ \textit{Divorce}(1)^* = \beta_{01} + \beta_{11}(\textit{ParentalDivorce}) + \beta_{21}(\textit{Family}) + \beta_{31}(\textit{Individual}) + \varepsilon_1$$

$$(2b)\ \textit{Divorce}(2)^* = \beta_{02} + \beta_{12}(\textit{ParentalDivorce}) + \beta_{22}(\textit{Family}) + \beta_{32}(\textit{Individual}) + \varepsilon_2$$

where Divorce(1)* and Divorce(2)* are unobserved latent dependent variables. We are able to measure only the observable dichotomies Divorce(1) and Divorce(2), where Divorce(1) = 1 if Divorce(1)* > 0 and Divorce(1) = 0 otherwise; a similar specification applies to Divorce(2). The model assumes that $E[\varepsilon_1] = E[\varepsilon_2] = 0$ and $Var[\varepsilon_1] = Var[\varepsilon_2] = 1$. We estimate $\rho = Cov[\varepsilon_1,\varepsilon_2]$, which measures the degree to which the residual for Sibling 1 is associated with that for Sibling 2. This SUR model also permits us to test the hypothesis Ho: $\beta_{11} = \beta_{12}$.

A more straightforward way to estimate the SUR models would have been to assign respondents from each subsample (GSS, SAF) to a separate equation. However, this is problematic because any differences across equations could simply be a product of the sample selection bias noted earlier. Our solution, above and beyond the two-stage selection correction described earlier, is to randomly assign each sibling in a pair to one of the two equations.

**Random Effects Models.** Another approach to studying the effects of parental divorce on the subsequent well-being of adult children is to use random effects models. Random effects models make the simple assumption that regression parameters will vary between sibling pairs due to unmeasured differences across the sample of sibships. The sibship-specific regression coefficients are treated as a "random" sample of coefficients drawn from some specified distribution; in our application, the distribution of random effects is assumed to be normally distributed. The random effects model assumes the following general form:

(3) $Y_{it} = \alpha + X_{it}\,\beta + Z_t + \varepsilon_{it}$

where Y is years of education completed for individual i of sibship t, X is a vector of dummy variables measuring family structure for individual i of sibship t, Z is a family-specific residual shared by both members in a sib pair, and ε is a residual term that has a zero mean, is uncorrelated with X and Z, and has a constant variance. The random effects model can be estimated based on the following:

(4) $Y_{it} - \rho\overline{Y}_t = (1-\rho)\alpha + (X_{it} - \rho\overline{X}_t)\beta + [(1-\rho)Z_t + (\varepsilon_{it} - \rho\overline{\varepsilon}_{it})]$
*where* $\rho = \sigma_Z^2/(\sigma_Z^2 + \sigma_\varepsilon^2)$.

If $\rho = 0$, then $\sigma^2_Z = 0$ and $Z_t = 0$ for all sib pairs and the model reduces to an OLS regression. If $\rho = 1$, then $\sigma^2_\varepsilon = 0$ and $\varepsilon_{it} = 0$, which means that there is no random error in the model (i.e., $R^2 = 1$). Values of ρ therefore measure the proportion of unobserved variation in the dependent variables that is shared by siblings within sibships.

Comparable random effects logistic regression models are estimated where divorce is the outcome. These models have the following functional form:

(5) $F(X_{it}\beta + Z_t) = \dfrac{1}{1+\exp(X_{it}\beta + Z_t)}$ *if* $y_{it} = 1$
*and*
$F(X_{it}\beta + Z_t) = 1 - \left(\dfrac{1}{1+\exp(X_{it}\beta + Z_t)}\right)$ *if* $y_{it} = 0$

where F(.) is the cumulative probability distribution, $X_{it}$ is again a vector of dummy variables measuring family structure for individual i of sibship t, and $Z_t$ is a family-specific effect shared by both members in sibship t. In this model, $Z_t \sim N(0,\sigma^2_Z)$. The statistic $\rho = \sigma^2_Z / (1 + \sigma^2_Z)$ measures the proportion of total variance that is explained by the sibship-level component. These models are estimated using generalized estimating equations (Liang and Zeger, 1993).

Random effects models offer three contributions. First, when analyzing sibling pairs or other clustered data random effects models yield more efficient estimates than do ordinary least squares. Second, unlike the SURs they allow us to generate a single set of parameter estimates for the entire sample, rather than results based on two subsamples. This should provide a more accurate appraisal of the effects of parental divorce, particularly when respondent divorce history is the outcome variable and the subsample sizes become somewhat small (N = 404 for each of the two subsamples in the SUR models). Finally, random effects models allow us to ascertain the extent of within-sibship variation that can be accounted for by differences in parental family structure.

# RESULTS

## Seemingly Unrelated Regression Models

Table 2 presents results from the seemingly unrelated regression (SUR) analyses of respondent education on family background and other variables. Sibling A and Sibling B refer to the randomized assignment of siblings into two subsets. As predicted, all models show that parental divorce has a negative effect on educational attainment, with step-parenting consistently having stronger effects than single-mother parenting. Also, the estimated effects of family structure do not change substantially from Models 1 to 2, suggesting that sociodemographic differences between respondents cannot account for the relationship between family structure of origin and educational attainment. Model 3 adds controls for within-sibship factors, including age differences and the gender composition of the sibship. This also had little effect on the coefficients measuring the effects of family structure on offspring education.

The SUR models allow us to ascertain whether parental divorce affects siblings differently. For each regression model we test the hypothesis $b_A = b_B$ for the effects of single-mother parenting and step-parenting on offspring educational attainment. The results of these tests are shown in the columns to the right of each regression model. Although the magnitude of some coefficients and their corresponding significance tests vary between Equations A and B none of these differences are statistically significant, which indicates no meaningful within-sibship variation in the effects of parental divorce on offspring education. In other words, with respect to educational attainment siblings react to the same divorce the same way. Taking all models into account, divorced-mother parenting on average reduces offspring educational attainment by up to three-fourths of a year (Models 2.B and 3.B). The figures for remarried divorced mothers are larger, with average educational attainment declining by up to two years (Models 2.A and 3.A).

A similar story emerges when considering how parental divorce affects the marital stability of adult offspring. Table 3 shows the results of the seemingly unrelated bivariate probit analyses of respondent divorce history on family structure of origin and other independent variables. In all of the B equations divorced-mother parenting significantly increases the likelihood that respondents dissolve their own marriages. Although the corresponding coefficients in the A equations are small and statistically insignificant, the differences across equations in the magnitude of the coefficients are themselves not significant. Perhaps the coefficients vary across models because of the reduced size of each sib-specific subsample. In any event, the equality of coefficients across models in Table 3 offers the same conclusion as do the SUR analyses of respondent education: the effects of parental divorce do not vary within the same family. Moreover, controlling for differences both within and between sibships does not affect the relationship between parental divorce and offspring divorce.

One surprising result suggested by Table 3 concerns the lack of a relationship between step-parenting and respondent marital stability. This is contrary to previous research (e.g., Glenn and Kramer, 1987; Wolfinger, 2000), which showed especially strong rates of divorce

TABLE 2

SEEMINGLY UNRELATED REGRESSION ESTIMATES OF RESPONDENT EDUCATION ON FAMILY STRUCTURE OF ORIGIN AND OTHER VARIABLES

| | MODEL 1 | | | | | MODEL 2 | | | | | MODEL 3 | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Sibling A | | Sibling B | | | Sibling A | | Sibling B | | | Sibling A | | Sibling B | | |
| | b | SE | b | SE | $b_A = b_B$ | b | SE | b | SE | $b_A = b_B$ | b | SE | b | SE | $b_A = b_B$ |
| Divorced mother | -0.42 | 0.32 | -0.79** | 0.30 | n.s. | -0.18 | 0.30 | -0.70* | 0.29 | n.s. | -0.19 | 0.30 | -0.70* | 0.29 | n.s. |
| Remarried mother | -1.55** | 0.51 | -1.26* | 0.55 | n.s. | -1.97*** | 0.49 | -1.39** | 0.53 | n.s. | -1.99*** | 0.49 | -1.41** | 0.53 | n.s. |
| Intact family | — | — | — | — | | — | — | — | — | | — | — | — | — | |
| Other | -1.50** | 0.27 | 0.66* | 0.27 | | -1.33*** | 0.26 | -0.28 | 0.26 | | -1.32*** | 0.26 | -0.30 | 0.27 | |
| Black | — | — | — | — | | -0.45 | 0.29 | -0.06 | 0.28 | | -0.48 | 0.29 | -0.07 | 0.28 | |
| White | — | — | — | — | | — | — | — | — | | — | — | — | — | |
| Other | — | — | — | — | | -0.36 | 0.36 | 0.34 | 0.39 | | -0.36 | 0.36 | -0.35 | 0.40 | |
| Parental education | | | | | | | | | | | | | | | |
| Not a H.S. grad | — | — | — | — | | — | — | — | — | | — | — | — | — | |
| H.S. graduate | — | — | — | — | | 0.80*** | 0.19 | 1.17*** | 0.19 | | 0.81*** | 0.19 | 1.18*** | 0.19 | |
| Junior college | — | — | — | — | | 1.55*** | 0.42 | 2.47*** | 0.43 | | 1.59*** | 0.42 | 2.48*** | 0.44 | |
| College grad | — | — | — | — | | 2.15*** | 0.28 | 2.49*** | 0.27 | | 2.15*** | 0.28 | 2.50*** | 0.27 | |
| Post graduate | — | — | — | — | | 3.32*** | 0.31 | 2.83*** | 0.31 | | 3.34*** | 0.31 | 2.85*** | 0.31 | |
| Data missing | — | — | — | — | | -0.54⁺ | 0.31 | -0.22 | 0.29 | | -0.53⁺ | 0.31 | -0.22 | 0.29 | |
| Birth cohort | 0.03*** | 0.01 | 0.04*** | 0.01 | | 0.01 | 0.01 | 0.01⁺ | 0.005 | | 0.01 | 0.01 | 0.01* | 0.005 | |
| Rural | — | — | — | — | | -0.51** | 0.15 | -0.35* | 0.15 | | -0.51** | 0.15 | -0.35* | 0.15 | |
| Catholic | — | — | — | — | | 0.54** | 0.17 | 0.35* | 0.16 | | 0.54** | 0.17 | 0.34* | 0.16 | |
| # of siblings | — | — | — | — | | -0.13*** | 0.04 | -0.19*** | 0.03 | | -0.13*** | 0.04 | -0.20*** | 0.04 | |
| Male | — | — | — | — | | — | — | — | — | | 0.16 | 0.14 | 0.17 | 0.14 | |
| Age difference between sibs | — | — | — | — | | — | — | — | — | | 0.01 | 0.02 | 0.02 | 0.02 | |
| Same sex siblings | — | — | — | — | | — | — | — | — | | 0.26⁺ | 0.15 | 0.13 | 0.14 | |
| λ | -2.34 | 1.93 | -0.81 | 1.94 | | -4.28* | 1.80 | -1.90 | 1.82 | | -4.27* | 1.81 | -1.99 | -1.83 | |
| Constant | 16.40*** | 0.86 | 15.91*** | 0.87 | | 15.72*** | 0.85 | 14.56*** | 0.87 | | 15.46*** | 0.86 | 14.38*** | 0.88 | |

⁺ $p < 0.10$ ; * $p < 0.05$ ; ** $p < 0.01$ ; *** $p < 0.001$

TABLE 3

SEEMINGLY UNRELATED BIVARIATE PROBIT ESTIMATES OF RESPONDENT DIVORCE HISTORY ON FAMILY STRUCTURE OF ORIGIN AND OTHER VARIABLES

| | MODEL 1 | | | | | MODEL 2 | | | | | MODEL 3 | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Sibling A | | Sibling B | | | Sibling A | | Sibling B | | | Sibling A | | Sibling B | | |
| | b | SE | b | SE | $b_A = b_B$ | b | SE | b | SE | $b_A = b_B$ | b | SE | b | SE | $b_A = b_B$ |
| Divorced mother | 0.07 | 0.28 | 0.55* | 0.27 | n.s. | 0.09 | 0.29 | 0.66* | 0.28 | n.s. | 0.04 | 0.29 | 0.66* | 0.28 | n.s. |
| Remarried mother | -0.26 | 0.47 | 0.04 | 0.59 | n.s. | -0.23 | 0.48 | 0.09 | 0.62 | n.s. | -0.32 | 0.47 | 0.20 | 0.62 | n.s. |
| Intact family | — | — | — | — | | — | — | — | — | | — | — | — | — | |
| Other | -0.28 | 0.24 | 0.32 | 0.23 | | -0.29 | 0.26 | 0.42$^{+}$ | 0.25 | | -0.35 | 0.26 | 0.46 | 0.25$^{+}$ | |
| Black | — | — | — | — | | -0.13 | 0.33 | -0.17 | 0.33 | | -0.13 | 0.33 | -0.17 | 0.33 | |
| White | — | — | — | — | | — | — | — | — | | — | — | — | — | |
| Other | — | — | — | — | | 0.47 | 0.39 | 0.39 | 0.40 | | 0.53 | 0.39 | 0.40 | 0.40 | |
| Parental education | | | | | | | | | | | | | | | |
| Not a H.S. grad | — | — | — | — | | — | — | — | — | | — | — | — | — | |
| H.S. graduate | — | — | — | — | | 0.06 | 0.17 | 0.30$^{+}$ | 0.17 | | 0.07 | 0.17 | 0.27 | 0.17 | |
| Junior college | — | — | — | — | | 0.34 | 0.37 | 0.55 | 0.42 | | 0.38 | 0.37 | 0.54 | 0.42 | |
| College grad | — | — | — | — | | -0.20 | 0.30 | -0.23 | 0.29 | | -0.18 | 0.30 | -0.23 | 0.29 | |
| Post graduate | — | — | — | — | | 0.34 | 0.32 | 0.49 | 0.33 | | 0.38 | 0.32 | 0.43 | 0.34 | |
| Data missing | — | — | — | — | | 0.12 | 0.28 | 0.13 | 0.27 | | 0.19 | 0.28 | 0.11 | 0.27 | |
| Age—age 1st wed | 0.09*** | 0.02 | 0.07*** | 0.02 | | 0.09*** | 0.02 | 0.07*** | 0.02 | | 0.10*** | 0.02 | 0.07*** | 0.02 | |
| Age—age 1st wed$^2$ | -0.002*** | 0.0003 | -0.001*** | 0.0003 | | -0.002*** | 0.0003 | -0.001*** | 0.0003 | | -0.002*** | 0.0003 | -0.001*** | 0.0003 | |
| Rural | — | — | — | — | | 0.05 | 0.15 | -0.12 | 0.15 | | 0.05 | 0.15 | -0.11 | 0.15 | |
| Catholic | — | — | — | — | | 0.03 | 0.15 | -0.02 | 0.16 | | 0.05 | 0.15 | -0.03 | 0.16 | |
| # of siblings | — | — | — | — | | 0.002 | 0.03 | -0.06$^{+}$ | 0.03 | | -0.01 | 0.03 | -0.05 | 0.03 | |
| Male | — | — | — | — | | — | — | — | — | | 0.13 | 0.14 | -0.16 | 0.14 | |
| Age difference between sibs | — | — | — | — | | — | — | — | — | | 0.02 | 0.02 | -0.03 | 0.02 | |
| Same sex siblings | — | — | — | — | | — | — | — | — | | 0.01 | 0.15 | 0.08 | 0.14 | |
| λ | -2.49 | 1.61 | -0.88 | 1.66 | | -2.48 | 1.68 | -0.96 | 1.73 | | -2.73 | 1.69 | -0.63 | 1.75 | |
| Constant | -0.25 | 0.73 | -1.06 | 0.76 | | -0.43 | 0.80 | -1.00 | 0.82 | | -0.56 | 0.81 | -1.02 | 0.84 | |

$^{+}$ $p < 0.10$ ; * $p < 0.05$ ; ** $p < 0.01$ ; *** $p < 0.001$

transmission for children reared by step-parents. Although puzzling, this finding is consistent within sibships and therefore accords with the other results presented here. It may be the product of a small sample size, given that only 2 percent of GSS/SAF respondents hail from step-parent families. Alternately, it may reflect the historical weakening of the transmission of divorce between generations (Wolfinger, 1999).

In additional models (not shown), we explore whether the negative effects of parental divorce vary by respondent gender, ethnicity, or presence of siblings. These analyses were only conducted for offspring educational attainment; preliminary analyses showed that the sample size was inadequate for the seemingly unrelated bivariate probit models of offspring marital instability. The analyses of educational attainment reveal only one statistically significant interaction, between respondent family structure background and offspring gender, ethnicity, and siblings: respondents of "other" ethnic background (neither African American nor white) and "other" parentage (not from an intact family, a divorced single-parent family, nor a divorced step-family) have significantly higher levels of educational attainment. For reasons described earlier in the paper, it is impossible to interpret results associated with "other" parentage.

Based on these results, the negative effects of parental divorce do not appear to vary by offspring ethnicity, gender, or presence of siblings, either between or within sibships. However, the random effects results presented below do not divide the sample between two equations and therefore offer a better test of demographic differences in siblings' reactions to parental divorce.

## Random Effects Models

Table 4 presents random effects estimates for the regressions of educational attainment on parental divorce and other independent variables. We first estimate a baseline model (1), in which birth cohort and the sample selection parameter ($\lambda$) are the only independent variables. This is compared to Model 2, which includes the family structure variables, in order to ascertain how much within-sibship variation in educational attainment can be explained by parental family structure. Model 3 includes a vector of respondent characteristics that do not vary within sibships, while Model 4 contains measures that may vary within sibships.

Model 2 shows a significant negative association between family background and educational attainment. Furthermore, the results are consistent in magnitude with those produced by the SUR analyses. Models 3 and 4 respectively control for differences between and within sibships. These results are also similar to their SUR counterparts, with the control variables having little effect on the relationship between family of origin and offspring educational attainment.

The other noteworthy result concerns the statistically significant rho ($\rho$) parameters, which indicate the amount of within-sibship variation in educational attainment not explained by the independent variables. Parental divorce accounts for very little of this variation: $\rho$ changes slightly, from 0.47 to 0.46, between Model 1, which omits the family structure variables, and Model 2. Although differences between sibships further reduce $\rho$ (Model 3), observed within-sibship differences do not (Model 4). These results show that factors aside from family structure play an important role in

TABLE 4

RANDOM EFFECTS ESTIMATES OF EDUCATION ON FAMILY STRUCTURE OF ORIGIN AND OTHER VARIABLES

| | MODEL 1 | | MODEL 2 | | MODEL 3 | | MODEL 4 | |
|---|---|---|---|---|---|---|---|---|
| | b | SE | b | SE | b | SE | b | SE |
| Divorced mother | — | — | -0.65** | 0.24 | -0.50* | 0.23 | -0.51* | 0.23 |
| Remarried mother | — | — | -1.44*** | 0.40 | -1.67*** | 0.37 | -1.70*** | 0.37 |
| Intact family | — | — | — | — | — | — | — | — |
| Other | — | — | -1.08*** | 0.20 | -0.81*** | 0.19 | -0.82*** | 0.19 |
| Black | — | — | — | — | -0.25 | 0.23 | -0.26 | 0.23 |
| White | — | — | — | — | — | — | — | — |
| Other | — | — | — | — | 0.01 | 0.29 | 0.02 | 0.29 |
| Parental education | | | | | | | | |
| Not a H.S. grad | — | — | — | — | — | — | — | — |
| H.S. graduate | — | — | — | — | 0.97*** | 0.15 | 0.98*** | 0.15 |
| Junior college | — | — | — | — | 1.95*** | 0.32 | 1.98*** | 0.32 |
| College grad | — | — | — | — | 2.28*** | 0.21 | 2.29*** | 0.21 |
| Post graduate | — | — | — | — | 3.03*** | 0.24 | 3.05*** | 0.24 |
| Data missing | — | — | — | — | -0.37+ | 0.21 | -0.37+ | 0.21 |
| Birth cohort | 0.04*** | 0.004 | 0.04*** | 0.004 | 0.01+ | 0.004 | 0.01+ | 0.004 |
| Rural | — | — | — | — | -0.41*** | 0.12 | -0.42*** | 0.12 |
| Catholic | — | — | — | — | 0.44** | 0.13 | 0.44** | 0.13 |
| # of siblings | — | — | — | — | -0.16*** | 0.03 | -0.17*** | 0.03 |
| Male | — | — | — | — | — | — | 0.18+ | 0.10 |
| Age difference between sibs | — | — | — | — | — | — | 0.02 | 0.02 |
| Same sex siblings | — | — | — | — | — | — | 0.19 | 0.12 |
| λ | 3.84** | 1.28 | -1.65 | 1.57 | -3.22* | 1.42 | -3.27* | 1.43 |
| ρ | 0.47*** | | 0.46*** | | 0.33*** | | 0.33*** | |
| Constant | 13.71*** | 0.56 | 16.19*** | 0.70 | 15.24*** | 0.67 | 15.02*** | 0.68 |

$^+p < 0.10$ ; $^*p < 0.05$ ; $^{**}p < 0.01$ ; $^{***}p < 0.001$

explaining sibling variability in educational attainment.

Table 5 presents the results of random effects logistic regressions of offspring marital stability on family structure of origin and other independent variables. The results accord with those already presented, so they will be described only briefly. Consistent with the SUR analyses of marital stability (Table 3), divorced-mother parenting increases the chances of offspring divorce whereas step-parenting does not. As for the random effects analyses of offspring education, family structure of origin accounts for only a small proportion of the variation in how siblings fare. Finally, controlling for observable differences both between and within sibships did not affect the relationship between parental divorce and offspring divorce.

Additional random effects models (not shown) test whether the consequences of parental divorce vary by respondent gender, ethnicity, or presence of siblings. These models have the advantage, in comparison to the seemingly unrelated regressions, of larger sample sizes and therefore provide better tests of potential demographic variation in the negative effects of parental divorce, both within and between sibships. However, very little

TABLE 5

RANDOM EFFECTS LOGIT ESTIMATES OF RESPONDENT DIVORCE HISTORY ON FAMILY STRUCTURE OF ORIGIN AND OTHER VARIABLES

| | MODEL 1 | | MODEL 2 | | MODEL 3 | | MODEL 4 | |
|---|---|---|---|---|---|---|---|---|
| | b | SE | b | SE | b | SE | b | SE |
| Divorced mother | — | — | 0.58 | 0.38 | $0.70^{+}$ | 0.39 | $0.71^{+}$ | 0.39 |
| Remarried mother | — | — | -0.23 | 0.71 | -0.23 | 0.71 | -0.21 | 0.71 |
| Intact family | — | — | — | — | — | — | — | — |
| Other | — | — | 0.02 | 0.32 | 0.08 | 0.34 | 0.09 | 0.34 |
| Black | — | — | — | — | -0.27 | 0.46 | -0.28 | 0.46 |
| White | — | — | — | — | — | — | — | — |
| Other | — | — | — | — | 0.75 | 0.51 | 0.75 | 0.51 |
| Parental education | | | | | | | | |
| Not a H.S. grad | — | — | — | — | — | — | — | — |
| H.S. graduate | — | — | — | — | 0.32 | 0.23 | 0.32 | 0.23 |
| Junior college | — | — | — | — | $0.88^{+}$ | 0.52 | $0.89^{+}$ | 0.52 |
| College grad | — | — | — | — | -0.42 | 0.41 | -0.42 | 0.42 |
| Post graduate | — | — | — | — | $0.79^{+}$ | 0.46 | $0.79^{+}$ | 0.49 |
| Data missing | — | — | — | — | 0.20 | 0.37 | 0.20 | 0.37 |
| Age – age 1st wed | 0.16*** | 0.03 | 0.16*** | 0.03 | 0.16*** | 0.03 | 0.16*** | 0.03 |
| Age – age 1st wed$^2$ | -0.002*** | 0.0004 | -0.003*** | 0.0005 | -0.002*** | 0.0004 | -0.002*** | 0.00 |
| Rural | — | — | — | — | -0.06 | 0.20 | -0.06 | 0.20 |
| Catholic | — | — | — | — | -0.04 | 0.22 | 0.03 | 0.23 |
| # of siblings | — | — | — | — | -0.05 | 0.04 | -0.05 | 0.05 |
| Male | — | — | — | — | — | — | 0.00 | 0.18 |
| Age difference between sibs | — | — | — | — | — | — | -0.01 | 0.02 |
| Same sex siblings | — | — | — | — | — | — | 0.07 | 0.19 |
| $\lambda$ | -4.00* | 1.87 | -3.46 | 2.32 | -3.62 | 2.36 | -3.50 | 2.37 |
| $\rho$ | 0.47** | | 0.46** | | 0.43* | | 0.43* | |
| Constant | -0.93 | 0.85 | -1.19 | 1.05 | -1.25 | 1.11 | -1.31 | 1.13 |

$^{+}p < 0.10$ ; $^{*}p < 0.05$ ; $^{**}p < 0.01$ ; $^{***}p < 0.001$

variation was observed. The only statistically significant interaction concerns educational attainment: male step-children from divorced families have lower levels of educational attainment than do their female counterparts. Since this result accords with neither prior research (e.g., Hetherington, 1993) nor our other analyses, we are cautious about attaching too much importance to it. Moreover, Type II error may be responsible given the large number of interactions we test.

## DISCUSSION

This paper has examined the effects of parental divorce on offspring marital stability and educational attainment using nationally representative sibling data from the General Social Survey (GSS) and the Survey of American Families (SAF). Our primary finding is straightforward: parental divorce affects siblings similarly. Moreover, the effects of parental divorce are uniformly negative. People from divorced families complete fewer years of school and are more likely to dissolve their own marriages than are people from intact families. Step-parenting further reduces offspring educational attainment but offsets the negative effects of parental divorce on offspring marital stability. Controlling for a variety of differences both within and between sibships does not attenuate the consequences of parental divorce, and we observed almost no variation according to respondent sex, race, or presence of siblings.

Given its gravity as a life course transition, it makes sense parental divorce affects siblings similarly. Divorce often disrupts almost every family routine (Wallerstein and Kelly, 1980), so it is not surprising that both members of a sibship may be affected negatively. Our results are also not surprising given the robust negative effects of divorce chronicled by earlier research. The children of divorce are no more or less likely to graduate from high school on time if they spend many or few years in a single-parent family, are old or young when first experiencing parental divorce, or live through multiple divorces (McLanahan and Sandefur, 1994). Similarly, recent studies show that the intergenerational transmission of divorce does not vary by sex, race, religion, or gender (Wolfinger, 1999, 2000, 2003b). Given that the consequences of parental divorce are so robust in other ways, it makes sense that all children within a family should be affected the same way. Our results also accord with what we know about the mechanisms responsible for the negative effects of parental divorce. Divorce affects offspring educational attainment largely through income and residential mobility (McLanahan and Sandefur, 1994), while the intergenerational transmission of divorce can probably be attributed to the lessons children learn about marital commitment (Amato and DeBoer, 2001). None of this should vary within sibships, so it is logical that siblings are affected similarly.

Although parental divorce can have large negative effects on offspring, it is only one of numerous factors that may do so. This is demonstrated by the fact that parental divorce accounts for only a small portion of the shared variance in siblings' educational attainment and propensity for divorce. Numerous other factors, most of them probably unmeasurable, also make a difference. This is why siblings can be so different even in the face of the large and homogeneous effects of parental divorce, a result that is in accordance with Andrew Cherlin's (1999) recent presidential address to the Population Association of America. Parental divorce sometimes has negative effects on offspring, but these are hardly inevitable given the number of other conditions affecting how children fare.

One limitation of this study concerns the data. The random effects models failed to account for considerable within-sibship variance in educational attainment and divorce history. Data on family processes and psychological well-being, both before and after the divorce, would permit more insight into whether the effects of family structure ever vary within sibships. Measures of pre-divorce well-being would be particularly useful, given that children's problems may be evident long before the divorce itself. In addition, the GSS/SAF measure of parental divorce, family structure at age 16, is relatively crude compared to those available in other surveys. More precise information might yield useful information on how siblings respond to divorce.

Although we have examined parental divorce as a socio-environmental life event among children, the sequela from parental family structure to offspring outcomes are affected by both social and biological factors. Our data do not allow us to measure the separate contributions of environmental, genetic, and gene-environment interactions that might account for why siblings of divorced parents are at (a common) greater risk of divorce and reduced educational attainment compared to siblings from intact families. We cannot rule out the presence of heritable factors such as depression and personality traits that may be shared among siblings (and indeed their parents), thereby predisposing them to similar adverse consequences ostensibly resulting from life events such as parental divorce (Jockin, McGue, and Lykken, 1996; McGue and Lykken, 1992). Further research is warranted to identify these heritable, shared biological factors.

Even with these limitations, the present study contributes to the literature by analyzing a large and nationally representative data set to provide findings on the nature of the long-term effects of divorce on adult children. Future research should attempt to discover why parental divorce affects siblings similarly. In the meantime, our results may be of interest to mental health professionals and others working with children adversely affected by divorce.

## APPENDIX A

### Coding of Variables

| | |
|---|---|
| Race | Set of two dichotomous indicators, each coded 1 if respondent is: Black, non-white/non-Black; white is the reference category |
| Religion | Coded 1 if Catholic, 0 if not Catholic |
| Family structure at age 16 | Set of three dichotomous indicators, each coded 1 if respondent family of origin was headed by a divorced single mother, a remarried divorced mother, or other; intact family is the reference category |
| Education of head of respondent parental family | Set of five dichotomous indicators, each coded 1 if family head: was not a high school graduate, had a junior college graduate, was a four-year college graduate, has a post-graduate degree, or if data are missing; not a high school graduate is the reference category. |
| Urbanicity | Coded 0 if respondent lived in a city 50,000 or more at age 16 or a suburb of a larger city, coded 1 if respondent lived in a town of under 50,000 or in a rural area |
| Sex | Coded 0 if female, 1 if male |
| Education | Continuous variable measuring years of completed education |
| Respondent divorce history | Coded 0 if no, 1 if yes |

| | |
|---|---|
| Age—age 1st wed | Continuous variable |
| Catholicism | Coded 1 if Catholic, 0 if not Catholic |
| Siblings are same sex | Coded 0 if no, 1 if yes |
| Absolute age difference between siblings | Continuous variable |
| Number of siblings | Continuous variable |
| Birth cohort | Continuous variable |

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